March 1, 1954

Dr. P. R. Edwards
Box 185
Chamblee
Georgia

Dear Phil:

I was very pleased to hear from you again, though it did somewhat prick my conscience that I have not been able to keep you better informed as to what was going on here. For the last couple of months, however, I have been preoccupied more with coli than with Salmonella. Under separate cover, I have made a point of sending you a copy of a progress report that may give you some idea of what has been going on. I am sorry to learn that you have been so overwhelmingly busy. Has there been any possibility of doing anything with the "Isecki" problem? Are there any strains that I ought to have sent to you and may have neglected to?

Unfortunately, I am not planning to go to the Pittsburgh meeting. Your letter was the first thing to make me feel especially sorry about it, as I would welcome an opportunity to review all of the stuff that has accumulated since our visit of one year ago.

I would, of course, have sent you the reprints that you asked for long ago except that I only received them a few days ago myself. You can count on my sending several copies of anything that may come out in the future.

When I have a chance to get back to research in Salmonella I will let you know, and I hope you can do the same for my benefit. The work on the genetics of the specificcoli types has been going rather slowly up to now, as Dr. Bernstein had been preoccupied with cleaning up the acriflavine story. There is a brief account of this in my report. To tell the truth, there are not many more details that might be given. Would the finding be of any practical use in your routine? I could mention that he found that cells killed with roccal behaved just like the live material. On the other hand, formalin will not do; it reacts directly with the dye on the one hand and on the other it makes the agglutination nonspecific.

We might possibly be even a bit more nostalgic for your southern climate except that we have had a remarkably mild winter here. Please give our best regards to everyone.

Yours sincerely,

Joshua Lederberg
Associate Professor of Genetics

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P.S. You will have seen Kauffmann's paper on transduction. I don't believe the story on somatic antigens is a true bill. At least there is no critical evidence that this is not simply a form variation that has nothing to do with transduction. Our story on the V antigen seems to me even stronger, but as you know I am mistrustful of this also.

I think you might be amused at some of the experiments I have been doing lately with single-cell isolation. Those abortive transductions that are represented by "trails" correspond to motile cells which at each division give a motile and a non-motile descendant. I have been following this directly under the microscope and it goes precisely as anticipated, besides being quite a bit of fun to separate the two individuals at each fission. So far I have been able to keep these "half-clones" going for about or up to 40 divisions.

J.L.